Reply to Referee #1

We sincerely appreciate the supportive and thoughtful remarks from the anonymous reviewer. Pleases find our point-by-point response below.

The numbers of line, equation, figure, table and section in red refer to the original manuscript, whereas in blue refer to the revised manuscript. The references cited in the responses are listed at the end.

The manuscript presents an in-depth description how three scattering models perform for the retrieval of non-spherical mineral dust particles from remote sensing measurements. It is of great interest to find a good representation of the irregular shape of dust aerosol particles in optical models. The figures are well prepared and the text is written in a clear, but rather descriptive manner. However, I have major concerns whether the content fits to a publication in Atmospheric Chemistry and Physics. In the following, I will describe my concerns.

1. The title states “Retrieval of microphysical properties …” And you retrieve microphysical properties, but we don’t know if they are correct. The manuscript merely describes the differences between two scattering models, namely the spheroid model and the irregular hexahedra (IH), and the calculations using spheres. The models are applied to synthetic data and to real world lidar measurements. However, the authors just report the retrieved microphysical properties without any validation. It remains unclear which of the non-spherical models retrieves more realistic values. The only validation briefly discussed is provided by an AERONET photometer retrieval which is as well based on the spheroidal model (stated very late in the text). Without a validation by independent observations, ideally with in situ measurements, I don’t see that the paper fills in the scope of ACP and might be better submitted to another journal.

A1: We believe that the quality of aerosol microphysical properties retrieved by inversion of optical measurements depends on two factors: (1) the capability of the scattering model to reproduce the measurements; (2) the performance of the scattering model in the inversion procedure. The factor (2) can be evaluated through numeric simulations as presented in many previous studies on numeric inversion of remote sensing data (Dubovik and King, 2000; Li et al., 2019; Müller et al., 2019). At the same time, we realize that the lack of independent observations for a closure validation is the main limitation of this study. Therefore, in the revised manuscript, we carefully stated this limitation (L845-L848, L957-L958) and explained the motivations of the retrieval simulations (L496-L502, L515-L523). Furthermore, we

(1) complemented the comparison between the model-simulated optical properties with previous lidar measurements (Sect. 3.1, Sect. 6.1);

(2) added a discussion section (Sect. 6.2) where a comparison of the retrieval simulation with previous studies is presented;

(2) added a discussion section (Sect. 6.3) where a detailed comparison of the real case retrievals with previous in situ/laboratory dust size, CRI and SSA results is presented, and where a more critical discussion of the comparison with AERONET retrievals is presented.

2. From an atmospheric science point of view, it is even more problematic, that you move in a circle while calculating the optical properties with the IH model and then invert these optical data to microphysical properties using the same model (Section 4). It is no surprise that the 3+2+3 inversion with the IH model leads to the best agreement if the optical data were calculated with this model before. Looking at an irregular hexahedron and describe it with a spheroid will of course lead to some differences when it comes to shape-dependent properties. But the driving question is how do we best describe the real dust particles.

   The same circle appears in Section 5 where you retrieve the microphysical properties with the IH model and then calculate with the same model (and the other models) back the optical properties like lidar ratio and depolarization ratio. This is not a real comparison.

A2: As mentioned in the previous answer, inverting synthetic optical data (Sect. 4) enables to separate the retrieval accuracy influenced by the "inverse performance" of a scattering model (e.g., the sensitivity, ill-posedness, condition number etc. of the inverse system) from that governed by the capability of accurately reproducing real measurements (this part is studied in Sect. 3), because: (1) the "true state" of aerosol microphysics is a priori known; (2) no modelling error is introduced to the synthetic optical data. Indeed, better results are expected from the 3 + 2 + 3 inversion than the 3 + 2 inversion. However, we hope to quantify the improvement brought by the PLDR measurements since more and more state-of-the-art lidar systems are capable of performing spectral depolarization measurements which have been proved to be informative in aerosol typing. Compared to previous studies concerning the PLDR inversion (e.g., Müller et al., 2013; Tesche et al., 2019), we try to comprehensively understand the PLDR inversion performance under different scattering models and wide ranges of microphysical states by simulations. At the same time, we noticed that it indeed makes less sense to use the spheroidal model to invert optical properties generated by the IH model, and vise versa. Therefore, we removed the relevant section (Sect. 4.3 in the original manuscript).
Section 5 allows to directly compare the retrieval differences caused by the use of different scattering models by inverting the same real lidar observations and substantiate conclusions drawn in the simulations. The purpose of recalculating from the retrieved microphysical properties back to the measurements is to visualize the fitting error. Sometimes large fitting error can be found due to modelling error or ill-posedness. So the magnitude of the fitting error is considered as a measure of inversion quality (e.g., Dubovik et al., 2006; Fedarenka et al., 2016; Lopatin et al., 2021)

3. The settings used for the calculations appear arbitrary (although to a certain degree reasonable) and are not based on literature or sensitivity studies. The three size distributions used 4.1 are arbitrary and I don't see why they should fit to the observations. There are numerous measurements of dust particle size distributions in literature. Why don't you base your assumptions on them? Or at least explain why you choose certain settings. The same holds

for the 3+2+3 data set. What about 3+2+1 or 3+2+2? Have you tested your results with only one depolarization ratio as input as well? It seems arbitrary or at least not explained why you have taken 3 depolarization ratios.

A3: According to your comments, we made a major revision to Sects. 3, 4. First, the setting of aerosol microphysical properties is now based on a more comprehensive survey of previous in situ and laboratory measurements of dust aerosols (Sect. 2.2): the effective radius covers the range 0.1-5 μm, and the CRI is based on the results of Di Biagio et al. (2019). Second, the reason for choosing the 3+2+3 data set lies in the consideration that it is a configuration well implemented in many state-of-the-art lidar systems, for example LILAS, while in the revised manuscript we tested the results with different combinations of depolarization ratios as input, for both the IH and spheroidal models. Please read the Sect. 4 of the revised manuscript for more details.

4. The work is not properly set into the context of previous literature. The discussion section which is commonly used to place the new findings in the scientific context does not contain any citation. Over wide parts in Section 3, 4, 5 & 6 the manuscript just describes the findings and does not discuss them. Why do we see a certain behavior? – Sometimes it is shortly mentioned. What is new? What was not known before? And many findings were known before, e.g., the comparison between spheres and spheroids. This prior knowledge was not used or is not properly acknowledged by the authors.

A4: We have been fully aware of this shortcoming and added more discussions combining previous studies and comparisons with findings from previous literatures, especially for the discussion section in the revised manuscript (Sect. 6). Given sufficient previous discussions on the contrast between the spheres and spheroids (e.g., Veselovskii et al., 2010), we reduced the contents related to the discussions on the spherical model and kept a small part for illustrating our consistency with the previous findings. Please read the revised manuscript for more details.

Specific comments on the sections

1. General
    1. Please explain abbreviations at first instance, e.g., AERONET, SAMUM, VIS, NIR.
    2. I still have troubles with the naming of the models: Sphere model or spherical model, Spheroid model or spheroidal model?
    3. While reading, I sometimes got lost in the results and lost the track why you are doing it. Or in other words, the storyline behind is sometimes not clear. It would be recommended to have a short introduction at the beginning of each section.

A1.1: They have been explained at first instance in the revised manuscript.

A1.2: Both “spheroidal” and “spheroid” were ever used by previous literatures. For clarity, we exploited the terms: “spherical model”, “spheroidal model” and “IH model” throughout the revised manuscript.

A1.3: A short introduction at the beginning of each section has been added.

2. Sections 2.1 & 2.2
    1. BOREAL was described in Chang et al., 2022 and it is good to give a short recap of it. By why do you not mention that it is a Maximum Likelihood Estimation. I find this fact rather central. What are the advantages and disadvantages of this method?
    2. L137: Which studies? You certainly need to consider several previous studies.
    3. What are the limitations of the spheroids and the IH?
    4. Here and also in the introduction, you completely omit the approaches which use the discrete dipole approximation (DDA). Why? They provide some realistic particle shapes.
    5. L173-175 Please provide a formula how you converted the diameter.
    6. How comparable are the results of the two particle shape models? How do you ensure to use the same shape distribution? The results probably depend (strongly or not) on the assumption of the shape distribution. Do you choose a sphericity for the IH which matches the shape distribution assumed for the spheroid model?

A2.1: We added that BOREAL was based on the maximum likelihood estimation in the revised manuscript. Compared to other retrieval methods based on constrained linear inversion, main advantages of BOREAL include the convenient way of accounting for a priori constraints of different types and improvement of retrieval efficiency (please see L123-L126 in the revised manuscript). However, it cannot provide effective constraints as particles become larger, which leads to underestimates of particle size and is known as its main disadvantage. On the other hand, since the limitation of the maximum wavelength, it is a universal limitation for the inversion of lidar measurements, regardless of the algorithms. Detailed explanation can be found in L530-L539 of the revised manuscript.

A2.2: For example, Mishchenko et al. (2002) and Dubovik et al. (2006) found the spherical model cannot reproduce the flat angular variation of laboratory-measured dust phase function at side and backward scattering angles. However, since we adjusted the structure of Sect. 2 and for the sake of clarity, we removed this sentence in the revised manuscript.

A2.3: The main limitation of the spheroidal model is that the computational accuracy deteriorates at the backward direction due to the limitations of the used geometric optical method, which could be one of the reasons causing the discrepancy between the simulated backscattering properties and real lidar measurements (L279-L286). For the IH model, since it is a quite recent model, we have not found any studies that report the limitation on the used computational methods; a potential limitation could be that it does not account for the surface roughness, but we are not able to find any publications discussing this influence on dust-size irregular particles, either.

A2.4: We noticed some modellings of dust backscattering properties using the DDA and deformations of spheroids and ellipsoids to characterize dust shape, for example, the study of Gasteiger et al. (2011). However, unlike the spheroidal and IH model, we cannot find their published database that are applicable to lidar inversion. As a completement, we briefly introduced more shape models and scattering computation methods other than the spheroidal and IH models, and discussed their limitations for lidar inversion applications in the revised manuscript (L252-L266).

A2.5: A formular has been provided as Eq. (11) in the revised manuscript.

A2.6: To make the particle size comparable, we use the volume-equivalent radius as the size descriptor for both spheroidal and IH models. The axis ratio distribution for the spheroidal model is fixed to the retrieval of the laboratory measurement of Volten et al. (2001). The degree of sphericity for the IH model is set to 0.71. We cannot ensure “the same shape distribution” between the two models because they represent particle shape and the shape distribution in quite different ways. We ever tried to retrieve the degree of sphericity of IH particles but failed due to the underdetermination. So, we fixed this parameter to a value that was advised to characterize dust particles by many studies (L312-L314), and compare the IH model with the spheroidal model. We agree that the change of degree of sphericity will lead to changes in optical properties but it is out of the scope of this study. We pointed out this perspective in the conclusion section (L959-L961).

3. Section 2.4
    1. Section 2.4.1 needs to be updated. There are several studies conducted in the last 15 years concerning the size distribution of mineral dust and the contribution of fine and coarse mode dust and its changes during transport process. Furthermore, it remains unclear if the morphology is expected to change with source region or not. Overall this section is more a lose collection of facts and needs to be straighten. What do you want to tell me? And how are these findings linked to your own research? With this general literature section, you do not explicitly motivate the choice of your size distributions in Sect. 4.1.
    2. L233 What are the a priori constraints for m_R?

A3.1: The Sect. 2.4.1 has been updated with more recent studies on dust size distribution and its changes during transport process (transformed to Sect. 2.2.1 in the revised manuscript). Furthermore, we explicated the size and source independence of dust aspect ratio distribution by citing the results of Huang et al. (2020) (L193-L197); we summarized previous in situ results of dust coarse-mode distributions with respect to different sources, transport times and measurement methods (Table 1). Then the choice of size distributions for the modified simulations in Sects. 3, 4 is based on these studies.

A3.2: The a priori value and a priori standard deviation for m_R are 1.5 and 0.5, respectively, as indicated in L230 of the revised manuscript.

4. Section 3
    1. Please add subsections to make it easier for the reader.
    2. You rarely set your results into context of previous findings. Especially in this basic section, many findings were known before or observed in similar studies. Reading your manuscript evokes the impression you are the first ones to observe this behavior of the optical properties. You may reduce some of text and put a stronger focus on the comparison between spheroids and IH.
    3. Furthermore, the findings are not compared against observations from laboratory and field measurements. It remains unclear whether the reported results are found in reality or if they are "just" an output of different models.
    4. It would be good to show three typical size distributions for three ranges of r_eff which you are discussing. You may add it as an additional subplot in Fig. 3. The constraint of V_t =1 (L315-316) will then become more visible.
    5. L317-319 Why do we see this behavior?
    6. L325-328 Why do we see this behavior?
    7. Fig 4f: It seems that the blue line goes to negative depolarization ratios for large particles. Can it really be the case?
    8. Fig 4: You may add a dashed line at r_eff = 1.25μm to enhance the link to Fig. 5+6.

A4.1: Subsections have been added.

A4.2: Following your advices, we made major revision of Sect. 3. Specifically, we (1) deleted the contents describing already well-known non-spherical scattering properties but less relevant to this study (e.g., angular variations of the phase matrix) and focus on backscattering properties instead; (2) more concentrated on the behaviors which influence the settings of following simulations and the interpretation of the retrieval results, with proper citations; (3) added two subsections discussing the influence of m_I spectral dependence (Sect. 3.2) and the sensitivity of depolarization measurements (Sect. 3.3).

A4.3: We added contents about comparison of the model-simulated EAE, BAE, LR and PLDR with real lidar measurements (summarized in Table 2 of the revised manuscript). The choice of size distributions and CRIs is based on the literature survey in Sect. 2.2. We drew the conclusion that the results simulated with IH model are better in the ranges of real measurements than the spheroidal model (L439-L441).

A4.4: We adjusted the setting of size distribution in the section of retrieval simulation: we abandoned the “three typical size distributions” and tested a wider r_{eff} range (0.1-5 µm) for better representing dust particles.

A4.5: The main reason could be that these particles have similar averaged projected areas if they share the same volume-equivalent radius, since the bulk extinction coefficient scales with the averaged projected area. However, we noticed that this expression is not rigorous enough because this behavior only holds for particles apparently larger than the wavelength. In addition, by rescaling the figure, we found the extinction coefficients of non-spherical particles are in fact higher than that of spherical particles with the increase of r_{eff}. Thus, we revised the expression as L369-L372 and converted Fig. 4 to Fig. 3 in the revised manuscript.

A4.6: We attribute the inherent reason for the PLDR difference between spheroidal and IH particles to the contrast of particle shape. Compared to the spheroidal particles, the IH particles are more irregular with asymmetric surfaces that could cause more complex inner reflections and more significant change of the polarization state between the incident and backscattered light. However, as the size parameter becomes smaller than the geometric optics region, the PLDR decreases and so does the sensitivity to the surface deformation, as can be seen from Fig. 2 of Gasteiger et al. (2011) (L399-L403).

A4.7: This could be an artifact in the spheroidal model when calculating large particles due to the degradation of computational accuracy. To avoid this confusion, we limited the maximum r_{eff} to 5 µm and replotted the figure as Fig. 4 in the revised manuscript.

A4.8: Figures 5-8 have been removed in the revised manuscript for the purposes explained in A4.2.

5. Section 4.1 & Fig. 10
    1. The comparison of the IH to the results of Hu et al., ACP 2020, was already shown by Saito and Yang, GRL 2021. Where are the differences and similarities to that comparison?
    2. There are certainly more multiwavelength observations of mineral dust. Why have you chosen these ones and no other ones? Recently, I found some new results in Gebauer et al., ACP 2024.
    3. TD, FD and BD should be written in each figure because the abbreviations are not self-explaining (here and in the following figures).

4. Which dust size distribution would you expect to be present in the field observations? The reasons for the fitting or not are not properly discussed.
5. The text from line 430 onwards already belongs to Section 4.2 or to an own subsection, but it is not really linked to the content of Section 4.1.

A5.1: We imbedded Sect. 4.1 into Sect. 3.1 which allows for a wider comparison with real measurements. We abandoned the microphysical property setting in the original manuscript and moved to r_{eff} covering 0.1-5 µm and CRI from Di Biagio et al. (2019). In addition, we considered more measurements as listed in Table 2 of the revised manuscript. The comparisons can be found in L411-L432 of the revised manuscript. The difference from Saito and Yang (2021) is that apart from PLDR and LR, we also compared EAE and BAE, and we put this single comparison into the context of the comparisons with other measurements so as to acquire some extended perspectives.

A5.2: We added observations of mineral dust from Sahara (Freudenthaler et al., 2009), Central Asia (Hofer et al., 2020), USA (Burton et al., 2015), as well as the dust transported to the USA from Sahara (Burton et al., 2015) to enrich the real measurement set to compare. They are summarized in Table 2 of the revised manuscript. We also added the results in Gebauer et al., ACP (2024), their measurements at 1064 nm are of great interest in our study. We appreciate your suggestion.

A5.3: Figure 10 in the original manuscript has been removed in the revised manuscript.

A5.4: We expanded the range of r_{eff} to 0.1-5 µm instead of the three VSDs selected in the original manuscript. A general finding according to the new VSD setting is that the VSDs of both spheroidal and IH particles can better fit the measurements for r_{eff} (between 0.4-1 µm) smaller than most in situ results (r_{eff} > 1 µm, Table 1). For the r_{eff} consistent with the ranges of most in situ results (r_{eff} > 1 µm), the IH model provides overall better fitting to the measurements than the IH model (L421-L422, L424-L426, L429-L432). We also mentioned in the revised manuscript that the discrepancy between the simulations and measurements can result from the limitations of the exploited setting of microphysical properties (L799-L810)

A5.5: We kept "the text from line 430 onwards" in the beginning of Sect. 4 while moved other parts of Sect. 4.1 to Sect. 3.1 as mentioned in A5.1. In addition, we explained the motivation of this section in more details in the beginning of Sect. 4, and stress that this section cannot provide a direct comparison of the retrievals derived by the two non-spherical models (we realized Sect. 4.3 makes little sense thus deleted this whole section in the revised manuscript), please see L499-L505 and L518-L526.

6. Sections 4.2 & 4.3
    1. Now, you introduce some quantities with a *, probably to distinguish true and retrieved parameters. However, the ^ for retrieved

parameters as introduced before is not used. Please be more consistent and if necessary add a short description at first instance.

2. Overall, I don't really see the significance of these two sections (4.2 & 4.3). You use one model (here IH) to generate particles and this model of course retrieves the results with less uncertainties. What is the benefit? What do we learn from it?
3. Fig 11, 13, 14: Please find a better representation of the x-axis. The CRI pairs in the current version are not very intuitive. One idea would be to add a two dashed lines to separate the 3 m_R blocks. In that way the correlation with m_I would be easier to grasp.
4. L475 Why do you see this behavior?
5. L504 The formulation is not clear.

A6: We largely modified the microphysical properties in Sect. 4.2 to make them less arbitrary: the r_{eff} from 0.1 to 5 µm covering the possible range from fine to coarse mode dust; the S_g of 1.95 which is in the middle of the range of in situ measurements (Table 1); the m_R from 1.4 to 1.6 and $m_{I,355}$ from 0.001 to 0.009 covering the ranges of the laboratory measurements by Di Biagio et al. (2019). The updated simulations are then based on the modified microphysical properties and the main conclusions keep unchanged. Moreover, we tested the retrieval performance when different combinations of δ measurements are accounted for. Since Sect. 4.1 in the original manuscript has been removed, Sect. 4.2 becomes to Sect. 4.1 in the revised manuscript.

A6.1: To be more concise and avoid redundant naming and characters, "a*" is no longer used in the revised manuscript. Instead, we describe a microphysical property by specifying it is "retrieved" from the inversion or "true" as the defined value if necessary.

A6.2: Briefly, the significance of Sect. 4.2 (Sect. 4.1) is to quantify how retrieval results are influenced by the inversion procedure, a key factor apart from modelling error to determine the retrieval quality. The basic idea of the retrieval simulation is similar to those remote sensing inversion studies (e.g., Torres et al., 2017; Veselovskii et al., 2002; Xu and Wang, 2015) where only one forward model was used. Instead, we conducted for both spheroidal and IH model, and focus on quantifying the improvement brought by spectral PLDR measurements. A more detailed description of the motivation is presented in L499-L505 and L518-L526. The general conclusion from Sect. 4.2 (Sect. 4.1) is that the incorporation of spectral PLDR, especially at 1060 nm, to the inversion dataset brings large improvement in microphysical property retrieval for both spheroidal and IH models, although to different extent.
However, we realized that Sect. 4.3 makes less sense. We had aimed to quantify the retrieval difference between the spheroidal and IH models when inverting the same synthetic measurements. However, as you comment, when a set of synthetic measurements is generated with one scattering model, it is of course that inverting it with another scattering model leads to worse results because of the modeling differences. We stressed this point in the revised manuscript (L518-L522). Accordingly, we removed the whole

section and think inverting the same real measurements is a better way to compare the retrieval difference between different scattering models.

A6.3: Figures 11, 13, 14 have been removed because of the change of microphysical property setting and the unclear representation (i.e., “CRI pairs”). Instead, we made Figs. 7, 8 to visualize the variation of retrieval accuracy against the true r_{eff} for IH and spheroidal models, Figs. 11, 12 to visualize the variation of retrieval accuracy against the true m_R and m_I for IH and spheroidal models, and Figs. S1, S2 which are the same as Figs. 11, 12 expect for a smaller r_{eff}. We hope the new figures will be more intuitive.

A6.4: The inherent reason is still unclear at the moment. We did not find such behavior in the monomodal retrievals. Thus, it can be because the presence of the fine mode increases the inversion instability. But we can identify this less accurate retrieval by checking the fitting error in practice. In this regard, fitting error can be treated as an indicator of retrieval quality.

A6.5: We acknowledge this expression is not rigorous because we did not really calculate the correlation coefficients. We meant to say the feature that if the spheroidal model derives a higher V_t than the IH model, the it usually also derives a higher r_{eff}, a lower m_R and a lower m_I. We have modified this expression and moved it to the discussion (Sect. 6.2, L840-L842).

7. Sections 4.4 & 4.5
    1. Much of the information presented in Tab. 3+4 is not used in the manuscript.
    2. And again, I don’t really see the value in it. It tells us, how far from the truth we get, when using a certain model or model configuration. If we create our data with IH, we get the best results with 3+2+3 IH. You show, how far we get from the simulated truth, if we describe it differently. But who knows how the mineral dust particles look like?

       All these comments underline the need of a clear motivation for your model comparison study. Because I think it is important to compare these scattering models and to point out the strengths and weaknesses. The focus of your study appears to me rather the comparison of these models than the retrieval of microphysical properties (as stated in the title). Because to do this retrieval you would need some more validation which retrieval fits best.
    3. L531: “the corresponding standard deviation of the noise distribution is a third of the maximum error” – Does it mean that you use a standard deviation of 3.3% for extinction and backscatter at 355 and 532?

4. I am a bit puzzled why you omitted the spheroids in this section. Please show the results for the spheroids as well or instead of the spheres.
5. L559-560 From the presented results of EAE and BAE you can not conclude on the limitations of the scattering models, because EAE and BAE seem to vary a lot with the assumed aerosol size distribution, which you state at the end of the sentence.
6. L561: What about only one depolarization ratio? Would you expect the same improvement or do you really need 3 depolarization ratios?
7. Section 4.5: Again, you don't link your findings to previous literature. What is new? What was studied already before? And I believe, if you do a careful literature search you'll find many conclusions going in the same or a similar direction. I am interested in your new conclusions on top of previous knowledge. Or at least to set your conclusions in the context of previous findings.

A7: We largely modified the contents of Sect. 4.4 (Sect. 4.2) since the setting of microphysical properties in Sect. 4.1 has changed. As a result, we modified Table 3 and moved it to Sect. 4.1 (as Table 4) in the revised manuscript, while we removed Table 4 in the original manuscript. Moreover, we replaced Fig. 15 with Figs. 13, 14 where the dispersions of the retrieved state parameters caused by introducing measurement noise are shown for different aerosol microphysical states, inversion measurement sets and scattering models. We drew more constructive conclusions on the influence of measurement noise when retrievals are derived with different scattering models from different measurement sets.

A7.1: Table 3 has been modified and moved to Sect. 4.1 (as Table 4) while Table 4 was deleted in the revised manuscript (see A7). A detailed discussion of Table 4 is presented in L603-L620.

A7.2: We agree that the "simulated truth" is never the "truth" because of the presence of modeling error. We also agree that the main limitation of this study is the lack of independent observations for the validation of retrieval results (L848-L849, L960-L961), and to this end, we extended the comparison with AERONET and added discussions about comparisons with previous in situ/laboratory measurements of dust microphysical properties, which are presented in Sect. 6.3 of the revised manuscript. However, the motivation of this simulation section (Sect. 4) is stated in **A6.2** and the point of Sect. 4.4 (Sect. 4.2) is to assess the influence of measurement noise since it is inevitable in a real measurement. The inversion of synthetic measurement set perturbed by random noise is a commonly used strategy in the development of retrieval methods (e.g., Müller et al., 2019; Torres et al., 2017; Veselovskii et al., 2002).

A7.3: Yes. To avoid any ambiguity, we presented the magnitudes of the maximum errors (L632) and standard deviations (L634) in the revised manuscript.

A7.4: The results related to the spheroidal model are now shown in Fig. 14 and are discussed together with those related to the IH model (L640-L651).

A7.5: According to the updated comparison results in the revised manuscript, the measurements of EAE and BAE can be reproduced by both models for r_{eff} between 0.1 and 5 µm, but compared to the spheroidal model, the IH model allows a r_{eff} range (r_{eff} > 1 µm) more consistent with the in situ results in Table 1. Thus, we modified this expression to L416-L422.

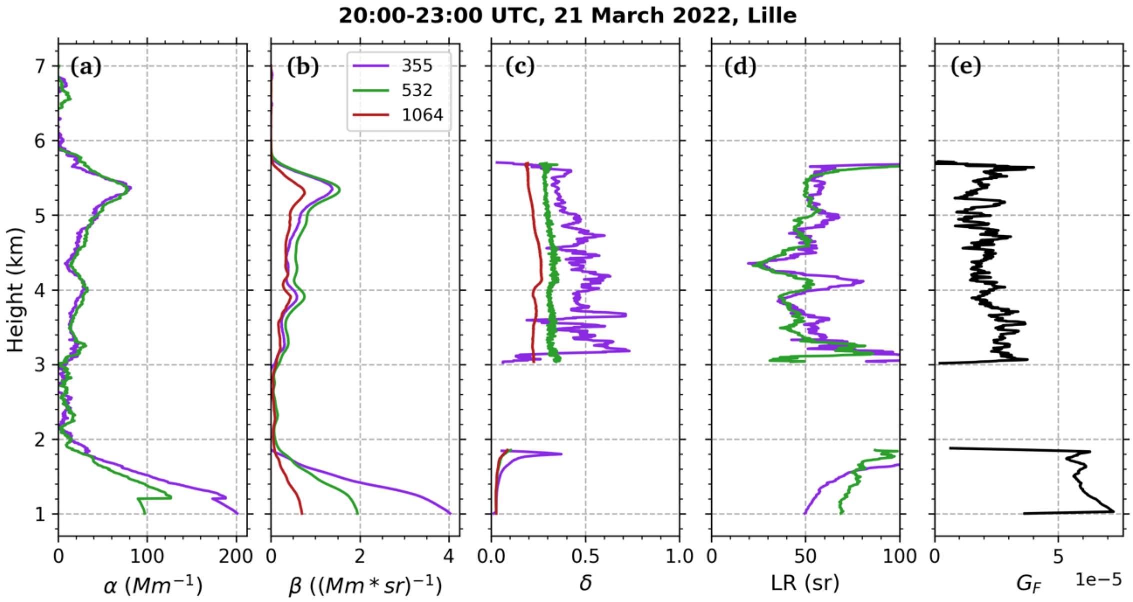
A7.6: We tested retrievals from measurements including different depolarization ratios, which is a main modification of this section (**A6**). Briefly, if only one depolarization ratio is included, δ_{1064} will bring more improvement than δ_{355} or δ_{532} due to the relatively large sensitivity to larger particles. However, including another δ_{355} or δ_{532} can further improve the retrieval of CRI and suppress the influence of measurement noise (L613-L615, L833-L835). So, the latest conclusion is that ($3\beta + 2\alpha + 2\delta$) with 1 δ at 1064 nm is the minimum configuration in reality.

A7.7: We have been fully aware of such insufficiency and more comprehensive discussions of the simulation results within the context of previous findings are presented in Sect. 6.1 and Sect. 6.2 of the revised manuscript.

8. Section 5.1
    1. L573: Do you really use the fluorescence measurements in the shown case studies to detect the dust layers?
    2. Fig 16: The data are not shown until 16 April 2019, 05:00 UTC as indicated in the figure caption.
    3. L594: What do these values tell me? Without an independent measurement or retrieval, I don't know which model fits best. m_R ranging between 1.45 and 1.68 it is a very wide range for mineral dust, the same holds for r_eff. You state, it was freshly emitted dust. However, a r_eff of 0.4-1.3 µm is much lower than your assumptions for fresh dust in Table 1.
    4. Please compare the retrieved CRI to the measurements of Di Biagio et al., 2019. What are their values for the Taklamakan and the Sahara? Are the retrieved CRI values reasonable?
    5. L605-610: The discussion is related to Fig 18 (whole layer) or Fig 19 (200 m layer)? Fig. 19 shows the size distribution and it so not so easy to get V_t and r_eff from it.
    6. Fig 19f: Please increase the scale for the depolarization ratio. The 3+2 spheroid results do not fit the measurements.
    7. L607-608: "Compared to the measurement error bars, all the scattering models are able to well fit the measurements." It is a very

dangerous statement, because it evokes the impression that everything fits. But this conclusion can not be drawn from your results. You use the optical data and invert them with a scattering model (IH) and then you apply the different scattering models to get to the optical data. And of course, this should fit somehow. But you are moving in a circle.

A8.1: As demonstrated by Veselovskii et al. (2022), dust particles have high δ_{532} and low fluorescence and they use the range $0.2 < \delta_{532} < 0.35$, $0.1*10^{-4} < G_F < 0.5*10^{-4}$ (G_F refers to the fluorescence capacity) to detect the presence of dust aerosols. We did not use the fluorescence measurements to identify dust aerosols for Case 1 because they are not available. However, the dust in Case 1 can be confirmed according to the study of Hu et al. (2020). The fluorescence measurements are available in Case 2. The values of G_F are within the range of dust, as shown in the following figure where the profile of G_F is plotted in panel (e):



The dust plume in Case 2 was further confirmed by PLDR, back trajectories and analysis of satellite images and synoptic conditions (L725-L739). To avoid the ambiguity, we deleted this sentence and did not show the fluorescence measurement in Case 2 in the revised manuscript.

A8.2: The data are shown until 16 April 2019, 00:00 UTC. The caption has been corrected.

A8.3: We found it is indeed a sketchy description that cannot provide helpful enough messages. To this end, we modified the interpretation of Fig. 18 (Fig. 17) in L1341-L1356, underscoring that the "variations of the inversions caused by different scattering models and input measurements are in line with our simulation results, indicating the results from ($3\beta + 2\alpha + 3\delta$) inversion are least affected by retrieval uncertainty" (L689-L697). A comparison of the retrievals with previous in situ dust size measurements is presented in Sect. 6.3 where the possible reasons for the systematic lower values of the retrieved r_{eff} are analyzed.

A8.4: The comparison of the retrieved CRI for Case 1 and Case 2 with the measurements of Di Biagio et al. (2019) is presented in Sect. 6.3 (L916-L922) of the revised manuscript.

A8.5: It is related to Fig. 19 (Fig. 18). The (V_t, r_{eff}) corresponding to each retrieved VSD is attached to Fig. 18a in the revised manuscript.

A8.6: Figure 19f (Fig. 18f) has been rescaled for easier reading. The (3+2, spheroid) result do not fit the measurements because the PLDR measurements are not inverted, indicating this configuration does not work well (L713-L715).

A8.7: With this sentence, we had hoped to state that the inverted measurements can be reproduced from the retrievals with an accuracy better than the measurement uncertainty, as an indicator of retrieval quality because we have seen large fitting errors can be the case in the simulation, as stated in L474-L475 of the original manuscript and in L590-L593 of the revised manuscript. However, we have realized such an expression can cause the ambiguity that "all measurements are well fitted". Thus, we modified the description in the revised manuscript (L710-L712).

9. Section 5.2
    1. Looking at Fig. 20, the dust layer seems quite homogeneous. Why do you limit your intensive optical properties to the tiny layer between 5.4 and 5.6 km height? What are the intensive optical properties for the whole layer (not shown)?
    2. The AERONET retrieval at 15:58 UTC, might it be affected by the feature at 9 km height (possibly an ice cloud) which is visible in Fig. 20?
    3. Why do you provide only the coarse mode r_eff from AERONET in Tab. 6? Especially, in case 2, the overall r_eff would be interesting to compare to your monomodal results. Then, it is not surprising that r_eff is higher for AERONET (L659). Please provide additionally r_eff for the whole AERONET size distribution.
    4. Table 5, case 1: Here report a layer height of 1 – 2.2 km, in Sect. 5.1 and Fig. 19, you use 2.0 - 2.2 km. Which layer do you use for the comparison in Tab. 6?
    5. To which layer height does the layer-averaged AERONET volume concentration correspond? Please add in L652 the layer height used for each case.
    6. I am bit puzzled what epsilon_fit represents in this section. Previously, you state that it quantifies how well the measurements are represented by the retrievals (L447). But here you have only the lidar measurements and then you do the retrieval. You don't have another independent quantity to compare your retrieval. Probably, you use the retrieved microphysical quantities and apply the same model to get the optical properties to report an epsilon_fit. However, it does not tell us, how well it fits in reality. Again, you're moving in a circle.

A9.1: We extended the retrievals to the layer 4.6-5.6 km, as shown in Fig. 22, and averaged the layer between 5-5.5 km for the visualization of VSD and SSA, as shown in Fig. 23. The intensive optical properties for the whole layer have been displayed (Fig. 20).

A9.2: We think it is less possible because we used the Level 1.5 retrieval product where the clouds should be well screened out according to Sinyuk et al. (2020) (L852-L853).

A9.3: The r_{eff} for the whole VSD retrieved by AERONET has been listed in Table 6. Indeed, the r_{eff} for the whole VSD in Case 2 is closer to the lidar retrievals due to the contribution of the fine mode.

A9.4: Table 5 is removed from the revised manuscript. The layer for Case 1 in Table 6 (Table 6 in the revised manuscript) is 1.5-2 km.

A9.5: The layer heights to which the AERONET columnar volume concentrations are averaged for each case have been specified in the caption of Table 6.

A9.6: The ϵ_{fit} in Table 6 represents the residual of the fitting to the measurements that take part in the inversion for both BOREAL and AEROENT retrievals. The definition of this parameter is expressed by Eq. (13) in the original manuscript, or Eq. (15) in the revised manuscript. To avoid the ambiguity, we removed this row in Table 6.

10. Sections 6 & 7
    1. At the beginning of discussion, you state it correctly, that BOREAL is able to reproduce the input measurements. But as long as we don't know the "microphysical truth", it is just a circle. You put some measurements in and get them out at the end. What do we learn from this? We know that the model works in the forward and backward direction.
    2. L695: "the $r$eff decreases to 11–12 µm due to the loss of sensitivity." What do you mean?
    3. L726-727: "All the retrievals fit the measurements well with a fitting error comparable with the measurement uncertainty." Again, I find dangerous to state it like this, because you do the retrieval on the retrieved quantities and then it is no surprise that it fits the original measurement. See also my previous comments.
    4. L728: If you consider the coarse mode r_eff from AERONET only. But to have a fair comparison, you should take r_eff from the whole AERONET size distribution.
    5. The data availability is not sufficient. Request to whom? Even better would be to publish the retrieval results separately as data set.

6. It is unusual to explicitly thank one of the co-authors in the acknowledgment section.

A10: The discussion section has been mostly rewritten by combining with previous findings as much as possible. In the revised manuscript, it consists of three subsections: Sect. 6.1 makes a further discussion of Sect. 3, aiming at demonstrating the capabilities of the IH and Spheroid models to mimic measured optical properties; Sect. 6.2 makes a further discussion of Sects 4 and 5, aiming at demonstrating the most preferable scattering model and measurement configuration for acquiring the best retrieval performance; Sect. 6.3 compares the real case retrievals in Sect. 5 with the corresponding AERONET retrievals and historical in situ measurements.

A10.1: We realized this paragraph makes less sense in terms of verifying the retrievals are capable of reproducing other optical properties that are not inverted. Thus, we deleted this paragraph. Comparisons of the real case retrievals with the corresponding AERONET retrievals and historical in situ measurements are presented in Sect. 6.3.

A10.2: It is a typo. We wrongly typed the values corresponding to "V_t" rather than "r_{eff}" in Table 6. We apologize for this.

A10.3: We are fully aware of the risk so this kind of statement no longer appears. Moreover, in order to be in line with the revised sections, especially the revised Sect. 6, the conclusion section is correspondingly modified.

A10.4: The comparison with AERONET concerning the r_{eff} for the whole columnar VSD is provided in Sect. 6.3 (L869-L873, Table 6) of the revised manuscript.

A10.5: The statement of data availability has been updated.

A10.6: The "acknowledgement" has been updated. The thank to the co-authors has been removed.

Technical corrections

- L185 reference appears twice.

It has been corrected.

- In Latex it is \AA ngstr\"o m exponent -> please use correct spelling

We spell it as "Angstrom" in the revised manuscript.

- L391 How does ...

The sentence has been deleted in the revised manuscript.

- Tab 3 caption: in parentheses ? The standard deviation is not given in parentheses. TD, FD, BD – please repeat the words in the table and not just the abbreviations.

The Tab 3 has been removed and we ensure that similar problems do not show in the revised manuscript.

- L579 Figure 16 shows the …

It has been corrected.

- L620 date is formatted differently here.

The format has been unified as: hh:mm UTC, dd Month yyyy.

References:

Burton, S. P., Hair, J. W., Kahnert, M., Ferrare, R. A., Hostetler, C. A., Cook, A. L., Harper, D. B., Berkoff, T. A., Seaman, S. T., Collins, J. E., Fenn, M. A., and Rogers, R. R.: Observations of the spectral dependence of linear particle depolarization ratio of aerosols using NASA Langley airborne High Spectral Resolution Lidar, Atmospheric Chem. Phys., 15, 13453–13473, https://doi.org/10.5194/acp-15-13453-2015, 2015.

Di Biagio, C., Formenti, P., Balkanski, Y., Caponi, L., Cazaunau, M., Pangui, E., Journet, E., Nowak, S., Andreae, M. O., Kandler, K., Saeed, T., Piketh, S., Seibert, D., Williams, E., and Doussin, J.-F.: Complex refractive indices and single-scattering albedo of global dust aerosols in the shortwave spectrum and relationship to size and iron content, Atmospheric Chem. Phys., 19, 15503–15531, https://doi.org/10.5194/acp-19-15503-2019, 2019.

Dubovik, O. and King, M. D.: A flexible inversion algorithm for retrieval of aerosol optical properties from Sun and sky radiance measurements, J. Geophys. Res. Atmospheres, 105, 20673–20696, https://doi.org/10.1029/2000JD900282, 2000.

Dubovik, O., Sinyuk, A., Lapyonok, T., Holben, B. N., Mishchenko, M., Yang, P., Eck, T. F., Volten, H., Muñoz, O., Veihelmann, B., van der Zande, W. J., Leon, J.-F., Sorokin, M., and Slutsker, I.: Application of spheroid models to account for aerosol particle nonsphericity in remote sensing of desert dust, J. Geophys. Res. Atmospheres, 111, https://doi.org/10.1029/2005JD006619, 2006.

Fedarenka, A., Dubovik, O., Goloub, P., Li, Z., Lapyonok, T., Litvinov, P., Barel, L., Gonzalez, L., Podvin, T., and Crozel, D.: Utilization of AERONET polarimetric measurements for improving retrieval of aerosol microphysics: GSFC, Beijing and Dakar data analysis, J. Quant. Spectrosc. Radiat. Transf., 179, 72–97, https://doi.org/10.1016/j.jqsrt.2016.03.021, 2016.

Freudenthaler, V., Esselborn, M., Wiegner, M., Heese, B., Tesche, M., Ansmann, A., MüLLER, D., Althausen, D., Wirth, M., Fix, A., Ehret, G., Knippertz, P., Toledano, C., Gasteiger, J., Garhammer, M., and Seefeldner, M.: Depolarization ratio profiling at several wavelengths in pure Saharan dust during SAMUM 2006, Tellus B Chem. Phys. Meteorol., 61, 165–179, https://doi.org/10.1111/j.1600-0889.2008.00396.x, 2009.

Gasteiger, J., Wiegner, M., Groß, S., Freudenthaler, V., Toledano, C., Tesche, M., and Kandler, K.: Modelling lidar-relevant optical properties of complex mineral dust aerosols, Tellus B Chem. Phys. Meteorol., 63, 725–741, https://doi.org/10.1111/j.1600-0889.2011.00559.x, 2011.

Gebauer, H., Floutsi, A. A., Haarig, M., Radenz, M., Engelmann, R., Althausen, D., Skupin, A., Ansmann, A., Zenk, C., and Baars, H.: Tropospheric sulfate from Cumbre Vieja (La Palma) observed over Cabo Verde contrasted with background conditions: a lidar case study of aerosol extinction, backscatter, depolarization and lidar ratio profiles at 355, 532 and 1064 nm, Atmospheric Chem. Phys., 24, 5047–5067, https://doi.org/10.5194/acp-24-5047-2024, 2024.

Hofer, J., Ansmann, A., Althausen, D., Engelmann, R., Baars, H., Fomba, K. W., Wandinger, U., Abdullaev, S. F., and Makhmudov, A. N.: Optical properties of Central Asian aerosol relevant for spaceborne lidar applications and aerosol typing at 355 and 532 nm, Atmospheric Chem. Phys., 20, 9265–9280, https://doi.org/10.5194/acp-20-9265-2020, 2020.

Hu, Q., Wang, H., Goloub, P., Li, Z., Veselovskii, I., Podvin, T., Li, K., and Korenskiy, M.: The characterization of Taklamakan dust properties using a multiwavelength Raman polarization lidar in Kashi, China, Atmospheric Chem. Phys., 20, 13817–13834, https://doi.org/10.5194/acp-20-13817-2020, 2020.

Huang, Y., Kok, J. F., Kandler, K., Lindqvist, H., Nousiainen, T., Sakai, T., Adebiyi, A., and Jokinen, O.: Climate Models and Remote Sensing Retrievals Neglect Substantial Desert Dust Asphericity, Geophys. Res. Lett., 47, e2019GL086592, https://doi.org/10.1029/2019GL086592, 2020.

Li, L., Dubovik, O., Derimian, Y., Schuster, G. L., Lapyonok, T., Litvinov, P., Ducos, F., Fuertes, D., Chen, C., Li, Z., Lopatin, A., Torres, B., and Che, H.: Retrieval of aerosol components directly from satellite and ground-based measurements, Atmospheric Chem. Phys., 19, 13409–13443, https://doi.org/10.5194/acp-19-13409-2019, 2019.

Lopatin, A., Dubovik, O., Fuertes, D., Stenchikov, G., Lapyonok, T., Veselovskii, I., Wienhold, F. G., Shevchenko, I., Hu, Q., and Parajuli, S.: Synergy processing of diverse ground-based remote sensing and in situ data using the GRASP algorithm: applications to radiometer, lidar and radiosonde observations, Atmospheric Meas. Tech., 14, 2575–2614, https://doi.org/10.5194/amt-14-2575-2021, 2021.

Mishchenko, M. I., Travis, L. D., and Lacis, A. A.: Scattering, Absorption, and Emission of Light by Small Particles, Cambridge University Press, Cambridge, 2002.

Müller, D., Veselovskii, I., Kolgotin, A., Tesche, M., Ansmann, A., and Dubovik, O.: Vertical profiles of pure dust and mixed smoke–dust plumes inferred from inversion of multiwavelength Raman/polarization lidar data and comparison to AERONET retrievals and in situ observations, Appl Opt, 52, 3178–3202, https://doi.org/10.1364/AO.52.003178, 2013.

Müller, D., Chemyakin, E., Kolgotin, A., Ferrare, R. A., Hostetler, C. A., and Romanov, A.: Automated, unsupervised inversion of multiwavelength lidar data with TiARA: assessment of retrieval performance of microphysical parameters using simulated data, Appl Opt, 58, 4981–5008, https://doi.org/10.1364/AO.58.004981, 2019.

Saito, M. and Yang, P.: Advanced Bulk Optical Models Linking the Backscattering and Microphysical Properties of Mineral Dust Aerosol, Geophys. Res. Lett., 48, 2021.

Sinyuk, A., Holben, B. N., Eck, T. F., Giles, D. M., Slutsker, I., Korkin, S., Schafer, J. S., Smirnov, A., Sorokin, M., and Lyapustin, A.: The AERONET Version 3 aerosol retrieval algorithm, associated uncertainties and comparisons to Version 2, Atmospheric Meas. Tech., 13, 3375–3411, https://doi.org/10.5194/amt-13-3375-2020, 2020.

Tesche, M., Kolgotin, A., Haarig, M., Burton, S. P., Ferrare, R. A., Hostetler, C. A., and Müller, D.: 3+2 + *X*: what is the most useful depolarization input for retrieving microphysical properties of non-spherical particles from lidar measurements using the spheroid model of Dubovik et al. (2006)?, Atmospheric Meas. Tech., 12, 4421–4437, https://doi.org/10.5194/amt-12-4421-2019, 2019.

Torres, B., Dubovik, O., Fuertes, D., Schuster, G., Cachorro, V. E., Lapyonok, T., Goloub, P., Blarel, L., Barreto, A., Mallet, M., Toledano, C., and Tanré, D.: Advanced characterisation of aerosol size properties from measurements of spectral optical depth using the GRASP algorithm, Atmospheric Meas. Tech., 10, 3743–3781, https://doi.org/10.5194/amt-10-3743-2017, 2017.

Veselovskii, I., Kolgotin, A., Griaznov, V., Müller, D., Wandinger, U., and Whiteman, D. N.: Inversion with regularization for the retrieval of tropospheric aerosol parameters from multiwavelength lidar sounding, Appl Opt, 41, 3685–3699, https://doi.org/10.1364/AO.41.003685, 2002.

Veselovskii, I., Dubovik, O., Kolgotin, A., Lapyonok, T., Di Girolamo, P., Summa, D., Whiteman, D. N., Mishchenko, M., and Tanré, D.: Application of randomly oriented spheroids for retrieval of dust particle parameters from multiwavelength lidar measurements, J. Geophys. Res. Atmospheres, 115, https://doi.org/10.1029/2010JD014139, 2010.

Veselovskii, I., Hu, Q., Goloub, P., Podvin, T., Barchunov, B., and Korenskii, M.: Combining Mie–Raman and fluorescence observations: a step forward in aerosol classification with lidar technology, Atmospheric Meas. Tech., 15, 4881–4900, https://doi.org/10.5194/amt-15-4881-2022, 2022.

Volten, H., Muñoz, O., Rol, E., de Haan, J. F., Vassen, W., Hovenier, J. W., Muinonen, K., and Nousiainen, T.: Scattering matrices of mineral aerosol particles at 441.6 nm and 632.8 nm, J. Geophys. Res. Atmospheres, 106, 17375–17401, https://doi.org/10.1029/2001JD900068, 2001.

Xu, X. and Wang, J.: Retrieval of aerosol microphysical properties from AERONET photopolarimetric measurements: 1. Information content analysis, J. Geophys. Res. Atmospheres, 120, 7059–7078, https://doi.org/10.1002/2015JD023108, 2015.